

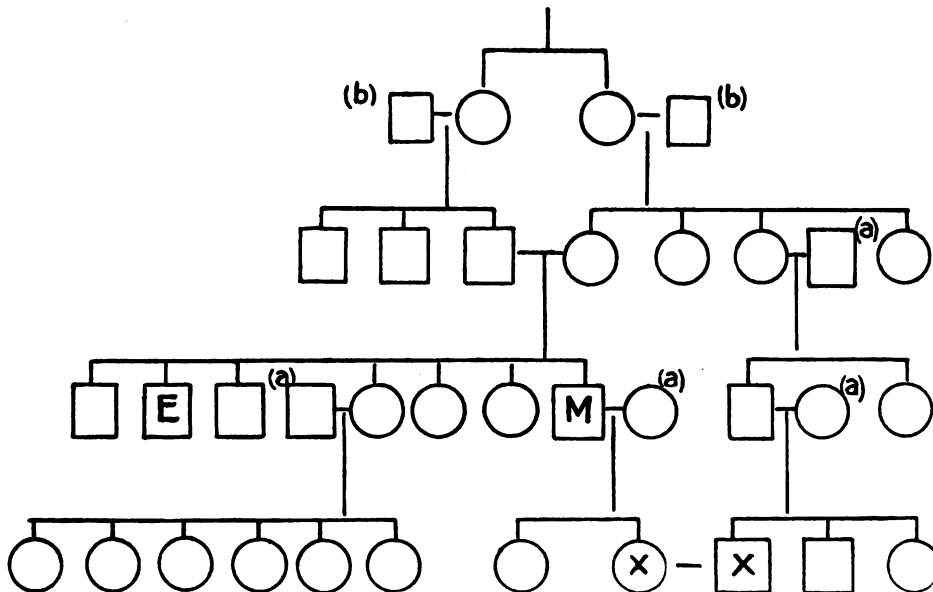
able woman) whose sister, by marrying her first cousin, betrayed the presence of defect in the strain in two of her seven children.

(5) That therefore we may expect that any children we may have will be free from defect themselves, and that, provided only that they marry into sound strains, the defect will disappear.

It seems to me that one or two questions of general interest and practical importance are in-

volved: How many generations free from defect are required before the "all clear" may be pronounced, or is our information insufficient upon this matter to warrant a conclusion? Can the assumption be accepted that if the strain is good on one side the children will be normal? What sources of information are available to the public upon this question?

I enclose a card with my name and address, and remain—Your obedient servant, X.



(a) coming from families which show no trace of epilepsy. (b) families little known.

Professor MacBride Replies

To the Editor, *Eugenics Review*

SIR,—In the last number of the REVIEW I wrote an account of de Beer's book on *Embryology and Evolution*. You took the somewhat unusual course of sending this account to Mr. de Beer himself before publishing it and of including the account and Mr. de Beer's reply in the same number. It is incumbent on me to make a short reply to Mr. de Beer.

He complains that I have not reviewed his book at all, and then goes on to say that I have accused him of making statements of which he is innocent. As Mr. de Beer states that one of his principal objects was to "dethrone" the recapitulatory interpretation of embryology, and as this interpretation is one of the principal supports of the theory of evolution and almost the

only support for the view that man is descended from simian ancestors, I thought it wise to expand the review into a general account of this theory in order to show readers of the *EUGENICS REVIEW* the solid ground on which it rests. As disputes between Mr. de Beer and myself turn largely on facts in a specialized science which could not be discussed in detail I had to give the general impression to be gathered from de Beer's arguments—and as he states that he is violently opposed to my views I do not think that I misrepresented his meaning. I admit with regret one slip. De Beer *did not* refer to the tri-tubercular theory of the origin of teeth in support of his view; all I can say is that if he had done so he would have made use of the strongest *prima facie* evidence in favour of his own view. Instead, he refers to the development of the teeth of the Dugong—a case which would be easy to deal with from my point of view if

there were space to enter into technical details. As to the other points, he refers (p. 103) to the "internal factors" or "genes" which control the appearance of embryonic characters and although on p. 35 he states that an animal when hatched is a larva and that primitively the embryonic stage is short: he then proceeds to adduce irregularities in embryonic development as arguments against recapitulation; but if, as I contend, the embryo is a modified larva, it is to the *larva* we must look for evidence.

Lastly, with regard to de Beer's errors—which show that in comparative embryology he has much to learn—it would be impossible to discuss them without entering into details; I

am chiefly interested in his statements about the larva of Echinodermata and primitive Vertebrata because that subject is one in which I have specialized. Nobody but himself has attached any importance to the speculations of Garstang on the subject—which were based on superficial examination not on thorough anatomical investigation, and which later research has shown to be entirely mistaken.

My object in writing the review was to show that de Beer has adduced no new evidence against recapitulation and that the evidence in its favour was steadily growing year by year.

E. W. MACBRIDE.

